On the Diminution of Light in its passage through Interstellar Space. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. The question whether light suffers any diminution in its passage to us from distant stars is by no means new, and has been hitherto answered provisionally in the negative. Thus Newcomb writes in his book, *The Stars: a Study of the Universe* (p. 231, edition of 1901):—

"The conclusion that the universe is finite rests upon the hypothesis that light is never lost in its passage to any distance, however great. This hypothesis is in accordance with our modern theories of physics, yet it cannot be regarded as an established fact for all space, even if true for the distances of the visible stars. . . . The hypothesis of a limited universe and no extinction of light, while not absolutely proved, must be regarded as the one to be accepted until further investigation shall prove its unsoundness."

2. Newcomb is here considering loss of light to the ether. But light may be lost in other ways; and it has been found that a very reasonable hypothesis of scattering of light by small particles in space will explain the following facts:—

(*a*) The ratios of the number of stars per magnitude tabulated by Kapteyn as far as the twelfth magnitude.

(*b*) The fact that when photographic exposure is prolonged in a ratio which ought to give stars fainter by *five* magnitudes, we only get *four* visual magnitudes. If there is scattering of light by small particles, it should be greater for photographic than for visual rays, according to Rayleigh's well-known law of λ^{-4}; and the puzzling failure of photographic exposure, which has been attributed provisionally to the behaviour of the plate, is thus explained as a celestial phenomenon.

(*c*) The ratios of the counts of stars for different exposures found at Greenwich, with a range from $20^{\text{sec.}}$ to $40^{\text{min.}}$

(*d*) The well-known fact that the total light of all the stars is comparatively small.

So far as I am aware, these are *all* the available facts of a general nature; and a simple hypothesis which accounts for them all is, therefore, worth attention.

3. First consider the simple case when all the stars are of the same brilliance, B, and are uniformly distributed through space. Let the distance of a star be r, and its apparent magnitude m. Then—

$$10^{-m \times 0\cdot4} = Br^{-2} \quad . \quad . \quad . \quad (1)$$

$$\therefore \quad dm \times 0\cdot4 \log_e 10 = 2\, dr/r \quad . \quad . \quad . \quad (2)$$

Hence the number of stars between magnitudes m and $m+dm$ is, if D be the number in unit volume,

$$4\pi D r^2 dr = 2\pi D \times 0{\cdot}4 \log_e 10. \quad r^3 dm$$
$$= D_1. \quad B^{\frac{3}{2}}. \quad 10^{m\times 0\cdot 6}. \, dm,$$

where D_1 is D multiplied by a numerical factor. And the total number of stars brighter than magnitude m and fainter than M is

$$N = D_1 B^{\frac{3}{2}} \int_M^m 10^{m\times 0\cdot 6} \, dm$$
$$= D_2 B^{\frac{3}{2}} \left[10^{m\times 0\cdot 6} - 10^{M\times 0\cdot 6} \right]$$

where D_2 is D multiplied by another numerical factor. When M is negative, as it certainly is for the brightest stars, the second term in the bracket is so small that we may neglect it and write

$$\log N = \text{const.} + m. \times 0{\cdot}6.$$

Since $10^{0\cdot 6} = 3{\cdot}98$, the total number should increase nearly 4 times for each magnitude,—a well-known result, which has often been quoted. The observed ratio is about 3·5, and the defect from 4·0 has been attributed to the fact that the stars are not of uniform brilliancy B, or to other causes.

4. But has it not been hitherto overlooked that if we conceive the existing universe made up of superposed systems of uniform brilliance, however many, the above law of increase in a geometrical progression with ratio 4 will hold for each system, and therefore for the whole? If the variation in B is not dependent on distance from our Sun, we can add up all the coefficients $D_2 B^{\frac{3}{2}}$ and still keep the common ratio. The appropriate expression is the integral

$$10^{m\times 0\cdot 6} \, dm \times \int B^{\frac{3}{2}} D_2 dB \quad . \quad . \quad . \quad (4)$$
$$= 10^{m\times 0\cdot 6} \, dm \times C,$$

so that the law of increase in number with magnitude will be the same as if the stars were all of a certain uniform brilliancy; and the increase per magnitude will still be 4 times if there is no absorption of light. The fact that Kapteyn finds, after a very careful discussion, the factor 3·5 (about) instead of 4 is thus evidence of absorption of light. If we admit the existence of absorption, the above demonstration no longer holds. We shall consider that the stars are distributed according to the law above indicated, and the hypothesis submitted for consideration can now be stated most simply by tabulating the facts as below.

TABLE I.

(1) True Mag.	(2) r.	(3) Log N.	(4) $\cdot 3r$.	(5) Visual Mag.	(6) Kapteyn.	(7) V—K.	(8) $\cdot 6r$.	(9) Photog. Mag.
0·0	0·063	5·50	0·02	0·02	9·64	+0·38	0·04	0·04
1·0	0·10	6·10	·03	1·03	0·68	+0·35	0·06	1·06
2·0	0·16	6·70	·05	2·05	1·76	+0·29	·10	2·10
3·0	0·25	7·30	·08	3·08	2·90	+0·18	·15	3·15
4·0	0·40	7·90	·12	4·12	4·00	+0·12	·24	4·24
5·0	0·63	8·50	·19	5·19	5·16	+0·03	·38	5·38
6·0	1·00	9·10	·30	6·30	6·36	−0·06	·60	6·60
7·0	1·58	9·70	·47	7·47	7·60	−0·13	0·95	7·95
8·0	2·51	0·30	·75	8·75	8·89	−0·14	1·51	9·51
9·0	3·98	0·90	1·20	10·20	10·28	−0·08	2·40	11·40
10·0	6·31	1·50	1·89	11·89	11·78	+0·11	3·79	13·79
11·0	10·00	2·10	3·00	14·00	13·47	+0·53	6·00	17·00

The *first* column gives the theoretical magnitude of the star on the assumption of no absorption of light, on Pogson's scale of magnitudes.

The *second* column gives the distance in units of that of a 6th magnitude star, calculated with Pogson's ratio of $10^{0 \cdot 4}$

The *third* column gives the logarithm of the number of stars, arbitrarily assuming 9·10 for the 6th magnitude to get agreement with the numbers given by Kapteyn in column 6 for stars between 5th and 6th magnitudes. See p. 54 of No. 18 of the Publications of the Astronomical Laboratory at Groningen, "On the number of stars of determined magnitude and galactic latitude," last column but one.

The *fourth* column gives the loss in visual magnitudes by scattering. The scattering in space is considered to be uniform, in which case the intensity of light I suffers diminution according to the law

$$dI = -k.Idr$$

or $$I = Ke^{-k_1 r} = K.10^{-k_2 r}.$$

$$\therefore \quad \log I = \text{const.} - k_2 r.$$

Since also $$\log I = \text{const.} - 0 \cdot 4.m$$

the loss in magnitudes is of the form $k_3 r$ where k_3 is a constant which, according to Rayleigh's well-known law, varies as λ^{-4}.

For visual magnitudes it was found by trial that the value $k_3 = 0 \cdot 30$ was suitable when r is expressed in units of the distance of a 6th magnitude star as in column (2).

For photographic magnitudes the value of k_3 will be greater. See remarks on the seventh column.

6. The *fifth* column gives the apparent visual magnitude on this assumption, being the simple sum of columns (1) and (4).

The *sixth* column gives Kapteyn's magnitude (visual) corresponding to the value of log N in column (3). The reference to his memoir is given above. He tabulates log N for galactic latitudes 0° to 20°, 20° to 40°, 40° to 90°, and then for 0° to 90°. To avoid raising the question as to the meaning of the Galaxy, the column for log N_{40}^{90} has been selected for use on the present occasion.

The *seventh* column shows the differences between our present assumption and Kapteyn's numbers. They are not very large between magnitudes 4·0 and 10·0, but run off at the ends. As regards the fainter stars, reasons will be given in § 19 for suspecting Kapteyn's magnitudes to be too bright. As regards the brighter stars, a reason can be assigned for the discrepancy in the existence of a solar cluster of about 60 stars of magnitndes 0 to 4; but it is too early to lay stress on this possibility.

7. The *eighth* column shows the photographic absorption in magnitudes. It was remarked above that this should be greater than the visual in the ratio of λ^{-4}. It is difficult to assign suitable exact values of λ, but easy to see what should be approximate values. If we choose the D line for visual magnitudes and G for photographic, we have almost certainly taken limits too wide. We should then have

$$\left(\frac{\lambda_D}{\lambda_G}\right)^4 = \left(\frac{59}{43}\right)^4 = (1\cdot38)^4 = 3\cdot6.$$

These limits are too wide and the number must be reduced. And for a reason given later it must be again reduced. We may consider a value 2·0 as not unreasonable.

8. Assuming, then, the factor 2, we get the eighth column; and by adding (8) and (1) we get the *ninth* column, which shows photographic magnitudes. Importance is attached to this column, since it explains a long-standing difficulty about photographic magnitudes, viz.:—How is it that by increasing exposure in a given ratio we do not get the effect equivalent to an increase of light in the same ratio? This has been a puzzle for years. It was reduced to quantitative statement by the present writer in 1905: see *Mon. Not.*, lxv. p. 755, etc. From an exhaustive study of the published Greenwich results it was found (see p. 775) that—

When the time of exposure is prolonged in the ratio of five star magnitudes the photographic gain is four magnitudes.

But on recently reviewing the life work of the late Mr. R. L. J. Ellery, it was found that he had stated this precise conclusion in 1892. (See *Mon. Not. R.A.S.*, lii. p. 265.)

The exactness of the correspondence between the results obtained in different countries, by different people using different instruments, plates, and methods, was so striking (taken also in conjunction with the fact that Dr. Schwarzschild had got very similar numbers), that attention was called to it in a brief note to the British Association at Dublin this year, and it was suggested that possibly this was a law of photographic action. Sir William

Abney had in 1893 announced the breakdown *for faint lights* of the law which would naturally be assumed (regarding exposure-time and increase of brightness as equivalent), and it seemed possible that the law might break down in a systematic manner of this kind. Against this was to be set the fact that the breakdown, as observed by Sir William Abney, was *not* systematic. (He was present at Dublin when the note above mentioned was read, and repeated his conclusion that the breakdown was not systematic.)

9. The present suggestion reconciles the facts. The definiteness of the result is due to the fact that the cause is not a photographic one, and Sir William Abney's breakdown does not apply. His breakdown may still be encountered for faint lights; but the light of the stars from (say) 5th to 10th magnitude, when concentrated by a 13-inch telescope, is *not* to be regarded as faint. It will obey the regular law announced by Sir William Abney long before he announced the breakdown in certain cases; and, accordingly, when we increase the exposure-time for such lights in the ratio of five magnitudes we *do* get five magnitudes fainter, and not four; but they are photographic magnitudes, and not visual.

For true magnitudes 5·0 and 8·0 the visual magnitudes are 5·19 and 8·75, difference 3·56; while the photographic magnitudes are 5·38 and 9·51, difference 4·13; and the ratio $4{\cdot}13/3{\cdot}56 = 1{\cdot}16$. If we take true magnitudes 6·0 and 9·0 we get, similarly, the ratio $4{\cdot}80/3{\cdot}90 = 1{\cdot}23$; and for true magnitudes 7·0 and 10·0 we get $5{\cdot}84/4{\cdot}42 = 1{\cdot}32$; mean of the three ratios 1·24.

The ratio changes throughout the table, but the discussion in *Mon. Not.*, lxv., was based on magnitudes in this part of the table; and it seems probable that the same applies to other discussions which led to the same result.

[Since this paper was written, the attention of the writer has been drawn to the valuable work of Dr. C. E. K. Mees and Mr. S. E. Sheppard, who have shown that the law of equivalence and intensity begins to break down when a certain exposure is reached, which Dr. Mees puts at about 15 minutes for such work as ours. The failure at 40 minutes, the longest exposure used in our stellar measures, is not serious. See *Investigations on the Theory of the Photographic Process* (Longmans), p. 214.]

10. It remains to justify the later portion of columns (5) and (8). Column (5) is in conflict with Kapteyn; but his fainter magnitudes depend on difficult observations, and are also subject to the criticism of § 19. It will therefore be sufficient for the present if column (8) is consistent with photographic results. Now the only way we have of testing these at present is by means of column (3). On the present hypothesis, length of exposure is a true test of photographic magnitude, and the number of stars ought then to increase with exposure, according to the law indicated by columns (3) and (8).

11. The most exhaustive counts of stars with different exposures

5

are those given in the Greenwich volumes, obtained from exposures of 20$^{sec.}$, 3$^{min.}$, 6$^{min.}$, and 40$^{min.}$. In the Astronomer-Royal's report for 1905 are given the figures in the first and last columns of Table II. The logarithms are taken in adjacent columns, and then the differences of these. Finally, the log (exposure) differences are divided by 0·4 to bring them to differences of photographic magnitude in the column adjacent to the differences of log N. It will be seen that the photographic magnitude increases altogether by 5·2 and log N by 1·34; ratio 3·9. Now 20$^{sec.}$ exposure corresponds to about a 9th magnitude star *visual*, which is between 8·0 and 9·0 true. Between 8·0 and 10·0 true the photographic

Table II.

Greenwich Counts of Stars.

Exposure. Sec.	Log.	Diff.	Photog. Mag.	Diff.	Log.	No. per Square Degree ÷4·05.
20	1·30				0·51	3·2
		0·96	2·4	0·68		
180	2·26				1·19	15·4
		0·30	0·8	0·12		
360	2·56				1·31	20·3
		0·82	2·0	0·54		
2400	3·38				1·85	70·0

magnitude increases by 4·28 and log N by 1·20, ratio 3·6. Between 9·0 and 11·0 true, the photographic magnitude increases by 5·60, and log N by the same, 1·20; ratio 4·7. The observed ratio 3·9 lies between 3·6 and 4·7, and is thus confirmed as nearly as we may reasonably expect with our rude approximation.

12. Another test of the hypothesis here advanced is afforded by the measure of the total light of all the stars. This is represented by the expression

$$\int 4\pi D r^2 dr \times B r^{-2} \times 10^{-r\times 0\cdot 12} = 4\pi DB \int 10^{-r\times 0\cdot 12} dr$$

$$= \text{const.} \times \left[10^{-r\times 0\cdot 12} \right],$$

the expression in brackets being taken within limits. Now, writing down the values of r for successive true magnitudes as in Table I., we have

Table III.

Total Light of the Stars.

True Mag.	r.	$10^{-r\times0\cdot12}$.	Diff.	Total No. Stars.	Diff.	Light of One Star.
0·0	0·063	·9826	·0099	1·3	3·9	·0025
1·0	0·10	·9727	·0157	5·2	15·7	·0010
2·0	0·16	·957	·024	20·9	62	·00038
3·0	0·25	·933	·038	83·2	248	etc.
4·0	0·40	·895	·055	331	987	
5·0	0·63	·840	·081	1318	3930	
6·0	1·00	·759	·116	5248	15640	
7·0	1·58	·643	·142	20890	etc.	
8·0	2·51	·501	·170	etc.		
9·0	3·98	·331	·156			
10·0	6·31	·175	·112			
11·0	10·00	·063	·051			
12·0	15·8	·012	·011			
13·0	25·1	·0010	·0004			
14·0	39·8	·0006				

13. The third column gives the value of the quantity which, taken between limits, represents the total light of the stars. If the limits are 0 and ∞ this quantity is unity. The fourth column shows the contribution of each successive true magnitude: we see that these contributions increase up to magnitude 8–9 and then fall off very rapidly. The fifth column gives the theoretical total number of stars, obtained by adding 4·62, the log of the number of square degrees on the whole sphere, to the numbers whose logarithms are given in the third column of Table I., and then finding the corresponding number. In column 6 the differences show the number belonging to each magnitude. Hence column 4 divided by column 6 shows the average light of a star between magnitude zero and 1, 1 and 2, 2 and 3, etc. These quotients, in column 7, diminish in the ratio 2·512, as they should. The light of a star of magnitude zero on this scale (on which the total light of all stars is unity) is thus about 0·004; in other words, the light of all the stars is about 250 times that of a star of zero magnitude. To fix the exact value we must take closer limits than whole magnitudes, for the average brightness of a whole magnitude is determined chiefly by the much more numerous faint stars. This calculation is easily made, and the number comes out about 240. Now, in vol. xiv. of the *Astrophysical Journal*, there is what is called "A rude attempt to determine the total light of all the stars," by Simon Newcomb. He found 600 times the light of a zero magnitude star, with a probable error of about one quarter of the whole amount. His estimate includes the Galaxy, which he found to be about 50 per

cent. brighter than the rest of the sky. The Galaxy has been deliberately excluded from our estimate (see § 6), and making allowance for this, his estimate is still double the number arrived at above; and though his probable error is admittedly large, the discrepancy is unsatisfactory unless it can be ascribed to some real cause.

14. One such cause we find in the variation in brilliancy of the stars. We have hitherto supposed them of uniform brilliancy, whereas we know they differ considerably. Let us imagine two superposed systems, each uniform in brilliancy and uniformly scattered, but one system being brighter than the other in the ratio of n^2 to 1. Then the stars of the brighter system which have the same *true* magnitude m will be n times as far away as those of the fainter. Let us suppose, purely by way of illustration, that the two systems contribute about equally to each *true* magnitude.

15. Now in the columns representing scattering in Table I. we are concerned with multiples of r direct. For these we are to substitute multiples of r_1 and r_2 where $r_2 = nr_1$. But the total absorption will not be altered if we keep the mean of r_1 and r_2 the same, *i.e.* if

$$r_1 + r_2 = 2r \qquad \text{and} \qquad r_2 = nr_1.$$

Thus
$$r_1 = 2r/(n+1) \qquad \text{and} \qquad r_2 = 2nr/(n+1).$$

Let us now see how these two systems contribute to the total light of the stars. They contribute equally to the true magnitude m, that is, to the magnitudes from m to $m + dm$. The number of stars for each system is proportional to $DB^{\frac{3}{2}}$ (see § 3). Hence

$$D_1B_1^{\frac{3}{2}} = D_2B_2^{\frac{3}{2}}$$

or
$$D_1 = n^3D_2.$$

Hence the total lights, which are in the ratio D_1B_1 to D_2B_2 are as n to 1, the inverse ratio of the distances which contribute to the same magnitude.

16. This general result also enables us to compare each system with the original single system. From equation $r_1 = 2r/(n+1)$ we see that the total light of the first system is $\frac{n+1}{2}$ times that of the original system; and similarly that of the second system $\frac{n+1}{2n}$ times. If then we wish to double the total light by our subdivision we have

$$\frac{1}{2}\left(\frac{n+1}{2} + \frac{n+1}{2n}\right) = 2$$

or
$$(n+1)^2 = 8n$$

or
$$n = 5{\cdot}8.$$

This shows that a hypothesis of this kind will explain the discrepancy between Newcomb's observed result and that found above; but at the same time the value obtained for n is too large to be admitted without question. A value $n=3$ would have been quite natural, for we know that the stars of type II, for instance, have parallaxes more than double those of type I; and when we allow for the further scattering of each type, we might easily divide the stars into two groups, one three times as far off as the other. But the value $n=3$ only gives us an increase in total light from unity to

$$\frac{1}{2}\left(\frac{3+1}{2}+\frac{3+1}{6}\right)=1\cdot3\,;$$

and even $n=4$ only gives $1\cdot6$; so that, with a probable value of n, we still find Newcomb's value in excess.

17. It thus appears that the variation in brilliancy of the stars will not much affect our results, and another reason must be sought for the discrepancy. One such reason is indicated in § 21 below; and of course our numbers are subject to revision. For the present it is only suggested that the coefficients ·30 for visual magnitudes and ·60 for photographic are apparently of about the right order of magnitude. Before any attempt is made to improve them, the hypothesis itself should be tested by further observations, which can easily be made, though not without some expenditure of time. For example—

(*a*) According to the present hypothesis of scattering of light, the anomalous results of increased photographic exposure are due essentially to the different wave-lengths of the light used for photography and for visual work. Therefore they should tend to disappear when photographs are taken in yellow light such as those taken with the Yerkes 40-inch and a colour screen: for such photographs, increased exposure should be much more closely the equivalent of our visual magnitudes. Possibly material is in existence which would settle this point.

(*b*) The Greenwich counts of plates with $40^{\text{min.}}$ exposure show that log N increases more slowly for the photographs of faint stars. This is as it should be on the present hypothesis, and the increase should be slower still as we proceed to fainter stars. This can be tested by comparing different (long) exposures with powerful instruments such as the large reflectors now in use. But it seems doubtful whether existing photographs could settle the point. The accidental errors will be very large unless the photographs are of the same region on the same night, and, of course, on similar plates. Care must also be taken that, in proceeding to deal with such very faint stars, we do not get a breakdown in the law of *photographic action* such as Sir William Abney has announced.

18. Other experiments will probably suggest themselves; but the facts quoted in the present paper represent a very large amount of work, and a hypothesis which reconciles them is entitled to con-

sideration until contrary evidence is forthcoming, based on a comparable amount of work.

19. Three points are still to be noticed. The first is the reason for suspecting the later portions of Kapteyn's lists already quoted; and the reason briefly is, that they give a value for the total light of the stars sensibly in excess of Newcomb's observed value. Take, for instance, his column log N_0^{90}, giving the average number of stars per square degree over the whole sky. There are about 42,000 square degrees in the sky, and so if we add 4·62 we shall get log totals over the sky. The number of stars of any given magnitude m may be taken as approximately the difference between the values of N for $m-0·5$ and $m+0·5$; and if the light be L(m) we have to sum the series

$$\begin{aligned}&N(0·5)\times L(0)+[N(1·5)-N(0·5)]\times L(1)+[N(2·5)-N(1·5)]\times L(2)+\ \dots\\&=N(0·5)[L(0)-L(1)]+N(1·5)[L(1)-L(2)]+N(2·5)[L(2)-L(3)]+\ \dots\end{aligned}$$

Now

$$L(0)-L(1)=L(0)[1-·3987]=0·6L(0)$$

and the succeeding factors form a geometrical progression with ratio $10^{-0·4}$. Hence to Kapteyn's value for log N(0·5) we must add 4·62 + log 0·6 = 4·40; and to succeeding half magnitudes 4·00, 3·60, 3·20, etc., as below.

Mag.	Log N.	Add.	Sum.	No.	Mag.	Log N.	Add.	Sum.	No.
0·5	6·18	4·40	0·58	3·8	10·5	1·23	0·40	1·63	42·7
1·5	6·72	4·00	0·72	5·2	11·5	1·68	0·00	1·68	47·9
2·5	7·24	3·60	0·84	6·9	12·5	2·12	9·60	1·72	52·5
3·5	7·77	3·20	0·97	9·3	13·5	2·55	9·20	1·75	56·2
4·5	8·29	2·80	1·09	12·3	14·5	2·97	8·80	1·77	58·9
5·5	8·80	2·40	1·20	15·8	15·5	3·38	8·40	1·78	60·3
6·5	9·31	2·00	1·31	20·4	16·5	3·78	8·00	1·78	60·3
7·5	9·81	1·60	1·41	25·7	17·5	4·16	7·60	1·76	57·5
8·5	0·29	1·20	1·49	30·9	18·5	4·54	7·20	1·74	55·0
9·5	0·76	0·80	1·56	36·3					
				166·6					491·3

It will be seen that up to the 18th magnitude we have 658 times the light of a zero star, already in excess of Newcomb's measure; and the series is still far from complete.

20. Secondly, it remains to suggest a *vera causa* for the scattering of light in space. It is not necessary to go beyond experience for this: we know that tiny particles of matter are being dissipated into space from the tails of comets and from the coronas of suns like our own. If they are dispersed under the influence of light pressure, they will be repelled from all bright bodies, and thus kept hovering in space except in so far as they fall into dark

bodies. It is at any rate conceivable that a balance has been reached in course of ages between the output from bright suns and the settling down of these particles on to dark suns or planets, and that the density of the particles in space is approximately uniform and constant. Since these particles are comparable in size with a wave-length of light, they are of about the right size to give the same kind of scattering of light to which Lord Rayleigh's law of λ^{-4} applies.

21. Thirdly, it remains to state a difficulty to which my attention has been drawn by Mr. H. C. Plummer since this paper was in type, viz. that we ought to be getting scattered light from the sky, in addition to the light of the stars. If the quantity thus received can be shown to be small, there is no difficulty at all; quite the contrary, for we might thus find an explanation of the defect from Newcomb's observed value for the whole light from the sky indicated in §§ 12–17. But it should be shown why the scattered light is small. For with an infinite universe the sky would be all over as bright as the average star if no light were lost. If light is merely scattered and not lost, the sky would still be as bright, though in a different way. There is, of course, no need to postulate an infinite universe for anything that precedes; the sum to infinity in § 12 is merely a convenient abbreviation. The present hypothesis does not abolish limits to the universe, it merely removes them further away than those which would otherwise have to be assigned to fit observed facts. But the hypothesis is incomplete without some examination of the fate of the scattered light. But this cannot be undertaken at the moment without unduly delaying the press. It seems probable that the hypothesis of simple scattering alone must be modified by the inclusion of actual absorption or obstruction of light by other particles too large to scatter it. Such obstruction would, however, be the same for all wave-lengths; and as one of the main objects of the present suggestion is to explain the discordance between visual and photographic results, scattering must have a large share in the total loss. For illustration, suppose the visual loss 0·30 to be made up of 0·15 obstruction and 0·15 scattering. Then if the photographic loss remains at 0·60, it must be due to 0·15 obstruction and 0·45 scattering; so that the ratio of photographic to visual scattering would be 3 instead of 2. From § 7 we see that this is not by any means impossible. But it is not intended to lay stress on these figures.

On the Inclinations of the Planes of some Spiral Nebulæ to the Galaxy. By H. Knox Shaw, B.A.

1. The measurements of spiral nebulæ contained in this paper were made at the suggestion of Professor Turner to test whether there is any connection between the planes of these nebulæ and that of the Milky Way, as seems to be the case with binaries and long-period variables (see *M.N.*, lxvii. p. 498 *seq.*, p. 352). The general assumption is made that each nebula is circular in form and appears elliptical owing to the inclination of its plane to the line of sight. This inclination is determined by measuring the axes of the ellipse which the nebula presents to us. It is of course impossible to tell in which direction the plane is tilted, and thus two poles are found for each nebula, either of which is equally likely to be the true one. They are each at a distance γ (measured in arc) from the nebula in the direction perpendicular to the major axis of the ellipse, where $\sec \gamma = \frac{a}{b}$; a, b being the measured major and minor axes of the ellipse.

2. Measurements were made of twenty-five spiral nebulæ from Isaac Roberts' photographs. Those most open, *i.e.* most circular in form, were excluded, for two reasons: firstly, because it was impossible to orientate them with any degree of certainty; and secondly, because a small error in the ratio of the axes, when they are nearly equal, produces a large error in γ. To avoid the labour of some rather heavy computation, the positions of the two ambiguous poles were found by means of a globe and protractor. This method seemed accurate enough for the purpose, as the photographs naturally did not admit of very precise measurement, the outline of the nebula being often very ill-defined, and dependent upon personal opinion. Two independent determinations were therefore made, at an interval of about a fortnight, in order that the second should not be in any way influenced by the first. It will be seen that in the case of some nebulæ the opinion as to their form changed considerably during the interval. The two determinations are throughout the paper shown bracketed, and in discussing the results the means of the two determinations are used.

3. In Table I. the measurements of the photographs will be found. The first column gives the name of the nebula, the second and third its R.A. and declination, the fourth its galactic latitude, the fifth and sixth the axes of the ellipse, the seventh the arc γ, and the eighth the angle θ, this being the inclination of the major axis to the hour circle, considered positive when the south end is turned in the direction of increasing R.A.